Now I can assure Mr. Tylor, from having often conversed with Dr. Martius on Brazilian topics, that his degeneration theory belonged to his earlier life, that afterwards he altered his opinions, and that the passage quoted by Mr. Tylor from Martius contained his latest conviction. Soon after the publication of his Ethnography he died at Munich. OSCAR PESCHEL

DR. MARTIUS found the rude natives of Brazil treating the hunting-ground of each tribe as common to all the tribesmen, but allowing each family to hold as its own freehold the ground which it had built huts on, or brought under tillage. It is not surprising that this ethnologist, comparing such a rudimentary form of the "village community" with its more artificial arrangements in ancient Europe, should have considered the Brazilian tribes to have arrived at an intermediate stage of the development of land-laws, above that of the lowest savages, and on the way to that of more civilised nations. Mr. Edkins, however, in his letter to NATURE, vol. x. p. 163, thinks that Dr. Martius should not have explained the origin of the Brazilian land-law in this obvious way. The suggestion which he offers in its place is, that inasmuch as the Chinese had in old times a highly artificial system of partitioning their village-lands among the heads of families, some of these Chinese are to be supposed to have emigrated to the Brazilian forests and introduced this system, which in course of ages decayed till nothing was left but the simple rule found by Dr. Martius. But is not the word "far-fetched" applicable to this argument? Sooner than allow the rude people of Brazil to have been human beings capable of adopting the simplest social regulation for their own evident benefit, Mr. Edkins sends half-way round the world for imaginary Chinese emigrants, to introduce, not the savage law itself, but a civilised law which, if broken down to its last remnant, might be reduced to the Brazilian level. And, one may go on to ask, where is it likely that the Chinese themselves got their law of village-lands, if it was not developed out of lower stages of the law of property, belonging to lower stages of civilisation? If Mr. Edkins would turn his great knowledge of Chinese matters to investigating the origin of Chinese institutions, I think he would contribute new evidence to the development-theory of culture. Mr. Edkins next brings forward the evidence of numerals in Polynesia as proof of degeneracy in civilisation. The fact that the word *mano* means 10,000 in the Tonga Islands, 4,000 in the Sandwich Islands, and 1,000 in New Zealand, he accounts for on the supposition that the highest number was the original meaning, but that it was lowered with a fall in civilisation. But he will, I think, on further examination be satisfied that the real reason has nothing to do with degeneration, but with the curious Polynesian habit of counting by twos, fours, and even tens. Thus *rau* and *mano*, which in New Zealand mean 100 and 1,000, come to mean in Hawaii so many fours, viz. 400 and 4,000; Mr. Edkins' own example from Ponape shows the same done with tens (see Hale's "Ethnography of Wilkes' Expedition"). Mr. Edkins also remarks that "the Polynesians formerly had a decimal arithmetic; now it has sunk in Australia to quaternary or quinary arithmetic." But the Australians are not of the same race as the Polynesians, nor is there the least reason to suppose that they were ever at a Polynesian level of culture. As the evidence of numerals has been introduced, it may be mentioned that both Australians and Polynesians use numerals derived from counting on the fingers. Thus the Polynesian *lima*, *i.e.* "hand," is the ordinary numeral for five, while the West Australian will say "the hand on either side and half the feet," meaning by this long expression the number 15 (see my "Primitive Culture," chap. 7). I may add that I have been trying for years to get any degenerationist to answer the argument from numerals of this very common class, which can only have arisen by development from the lower stage of counting on the fingers, and which therefore prove savage tribes to be capable of independent intellectual development.

The *Quarterly Review* argument from the recent discoveries of Dr. Schliemann in the ruins he considers to be of Troy, merely shows that low barbarians may build on the ruins of towns previously inhabited by more civilised nations. This often happens, and can hardly be held to prove that the higher civilisation existed in the world before the lower.

As to the observations (vol. x. p. 163) of Mr. Hyde Clarke on affinities which he believes to exist between languages of Brazil and of the Old World, I cannot make any answer, not having seen any comparative vocabularies on which such an opinion could be founded.

EDWARD B. TYLOR

Photographic Irradiation

FOR the purpose of determining whether any sensible amount of the photographic irradiation surrounding the image of a bright object could be traced to an action taking place within the thickness of the collodion film, I some time ago tried an experiment in many respects similar to that detailed by Mr. Aitken in your last number (vol. x. p. 185). A piece of cardboard with four parallel narrow openings, each some 12 in. long, was hung against the glass roof of a photographic studio so as to be projected against the background of a bright sky. One of the slits or openings was covered with a piece of red glass, another was glazed with blue glass, the third was left entirely uncovered, and the fourth was covered by a piece of thin tracing paper. The slits in the cardboard screen were carefully focused, and over-exposed photographs were taken with a camera in which no stops were used. Upon the collodion film and immediately in contact with it was laid a piece of platinum foil quite thick enough to be perfectly opaque. The camera was so placed that the images of the slits fell partly upon the platinum foil and partly upon the collodion film. I have now before me two of the plates, each taken with an exposure of five minutes. The first was coated in the ordinary manner with a single collodion film, but the other was coated three times successively with collodion, so that the film was rendered very thick; but the eating in or encroachment of the photographic images of the slits under the platinum foil is hardly perceptible in either plate; indeed, I feel that I cannot say with certainty whether there is any encroachment of the image proper, though there are very marked brush-like extensions from the ends of the images, as well as a cloudy semicircular field symmetrical with the end of each image, evidently arising from reflections from the back of the plate. At first sight the brush-like semi-opaque extensions might be taken for the ordinary photographic irradiation eating under the platinum foil; but on more closely examining the ends of the images, the hazy opacity is seen to extend farther in some directions than in others, and to be broken up in some cases into five or six little streams or brushes. The decrease in the opacity of the brushes is also less uniform than the decrease in the opacity of the ordinary irradiation border. The brushes extend to a distance of about ·02 in. under the edge of the platinum foil.

I do not at present see my way to devise an experiment which would determine what is the cause of these little brushes, nor have I at present had an opportunity of repeating a similar experiment with the dry-plate process; but the brushes have the appearance to me of having been produced by streams in the delicate film of liquid which must extend under the platinum, streams which probably carry with them little masses of light-altered silver, that are soon deposited or strained out in the spongy tissue of the collodion.

If the spreading action under the platinum foil were caused by light dispersed within the thickness of the collodion, one would expect such action to take place symmetrically around the place where the bright image is cut off instead of being broken up, as I have described, into bundles or brushes. On the other hand, slight differences in the texture of the collodion, or minute inequalities on the edge of the platinum foil, might cause the streams in the liquid film to move more easily at one point than at another.

I should be glad to be informed what was the distance of the opaque bar from the collodion plate in Mr. Aitken's experiments, and whether there is not any photographic trace of defraction bands, owing to the bar not having been in focus; possibly the presence of these may account for the apparent difference in our results. It will be seen that the experiment which I have described points to the same conclusion as that formerly announced by Lord Lindsay and myself, viz. that the inner photographic diffraction edge is chiefly due to the imperfection of the instrument producing the image, chief among which is to be counted the aberration of oblique pencils.

A. COWPER RANYARD

MR. AITKEN'S observations on photographic irradiation in NATURE, vol. x. p. 185, are confirmed by many experiments I have made. I spent a long time in efforts to get rid of irradiation in bromide of silver films, one of the results of which I stated in a former note to NATURE (vol. x. p. 63). There is the most striking difference in the behaviour of films containing iodide of silver only to those containing the bromide alone, the latter, especially when dry, giving much greater irradiation; and the difference is again complicated by the addition of certain substances (notably albumen) to the film in the course of preparation. As my experi-

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ments were mainly with dry plates, I will leave out of question the forms which the phenomenon may assume in wet-plate photography, and summarise the results of hundreds of experiments with dry plates iodised, bromo-iodised, and bromised.

With a simply bromised film the amount of irradiation is extreme. The film is very translucent and the irradiation is of two kinds, that caused by reflection from the back of the plate being by far the most extensive, but remediable by the usual expedient of coating the back of the plate with red or black colour, while the form noticed by Mr. Aitken is perhaps partially inherent in bromised films, but to a much greater degree dependent on the nature of the pyroxyline. Two samples of pyroxyline made at different temperatures, and treated in precisely the same manner, differ so much, that while one will, with the coloured backing, give scarcely a perceptible degree of irradiation, the other will develop it to an extent which no backing, nor even tinting the film with the aniline reds, will obviate. The former is generally a compact, lustrous film, scarcely to be distinguished from the glass itself, while the other (both being used without preservative solution) will give a dull and dusty-looking surface, only capable of reflecting at very small angles. If with the latter a strip of blackened wood be laid on the film so as to cut across the lightest portions of the image thrown on it by the lens, the effect of the light will be found to spread behind the strip of wood, sometimes to the extent of a centimetre; but I have never noticed the sharp limitation of this form of irradiation which Mr. Aitken observes, and which probably depends on the wet state of the film. It is clearly, as he supposes, an agitation which is set up in the film, and which depends for its propagation amongst the surrounding molecules upon a kind of chemical transparency in the film holding the bromide of silver. That this is to a great extent true is shown by two experiments: (1) a film which, in its simple state, gives considerable halation, will, when coated with albumen, especially if coagulated with nitrate of silver, give none at all, or very little, though the ocular transparency is rather increased than diminished by the albumen; (2) an emulsion prepared by exposing it to the action of nitrate of silver until it becomes structurally decomposed, and highly charged with bromide of silver, shows absolutely no irradiation under any circumstances even if the glass be not backed, and no kind of preservative used. The film in this case resembles unbaked porcelain in its whiteness, entire want of lustre, and in opacity, and the molecules of bromide of silver are more than usually free from any restraining influence which a preservative might be expected, reasoning from the usual action of the albumen, to exert. In these two cases of extreme translucency and opacity of the film there is almost an equal freedom from the phenomenon in question.

In the old albumen process with translucent films the irradiation is imperceptible, and in the collodio-albumen, where the film of albumen is allowed to remain on the collodion, it is almost so; but in this case, as in all cases where the film is charged only with iodide of silver, there is another element which complicates the action. The bromide of silver is reduced *in situ* while the iodide requires a supply of silver from the developer from which to build up the image, in the one case the deposition being by reduction, in the other by accumulation. This alone would account for a wide difference in respect to irradiation, but will not account for all, as is proven by the diverse results obtained from different bromide films, due to the varying structure of the material which holds the bromide in place.

What Mr. Aitken calls "molecular irradiation" (and which is not by any means the harmless thing he considers it in regard to artistic photography any more than to scientific) is unquestionably the great enemy of all photographic precision. It seems, however, to be complicated with what I have been obliged to call structural irradiation, alluded to above, and depending, as I have said, on the mechanical rather than the chemical condition of the pyroxyline of which the bulk of the film consists. The subject yet demands much investigation, of a purely empirical character, in order to determine the quality of vehicle for carrying the sensitive salts, neither chemical analysis nor chemical analogy affording any indication of the true cause of the difference between the two qualities of pyroxylene I have noted, nor do they, so far as I am aware, account for the difference between the action of collodion and albumen.

W. J. STILLMAN

Altenburgh Gardens,
Clapham Common, S.W., July 13

OBSERVATORIES IN THE UNITED STATES*

II.

LIEUT. M. F. MAURY was placed in charge of the new U.S. Naval Observatory, and entered on his duties with zealous purposes. He proposed in 1846 the immense astronomical work of a more extensive and precise cataloguing of the stars than Bessel's "Zone Observations" or Struve's "Dorpat Catalogue." Valuable results of the scheme, so far as it could be entered on, by the observations of Profs. Coffin, Walker, Yarnall, Hubbard, Keith, Major, and Ferguson, and Lieutenants Almy, Maynard, Muse, and others, have been lately reduced and published.

Two events marked this early part of the history with still more importance. Walker, in 1846, proved that the new planet Neptune, just then discovered by Leverrier, had been catalogued as a star by Lalande in his "Histoire celeste" in 1793; and Walker, with Lieutenants Almy and Gilliss, was the very first to use, in 1846, the new discovery of the telegraph to determine differences of longitude. The identification of Neptune with Lalande's star gave astronomers, in determining the new planet's orbit, the use of observations made fifty-two years before. It gave the *American Nautical Almanac* two years earlier ephemerides for the mariner. It brought the observatory into prominence. The superintendency of Maury extended from 1845 to April 26, 1861, when he suddenly left the city to join the cause of the South.

In 1861 Lieut. J. M. Gilliss was at length placed in charge. He re-established and vigorously pressed forward astronomical work as well as the duties of the "Hydrographical Office," a title which had been added to that of the Naval Observatory. After his very sudden death, his successor, Rear-Admiral C. H. Davis, carried forward the astronomical work with that eminent success which had been guaranteed by his previous astronomical tastes and occupancy on the Coast Survey and as superintendent of the *Nautical Almanac*. Rear-Admiral B. F. Sands, succeeding him in the year 1867, has most efficiently improved the opportunities of a longer superintendency to inaugurate and carry forward some of the most important astronomical operations of the day. The phenomena of the total eclipses of 1869 in the United States and of 1870 in the Mediterranean countries were closely observed.

Beyond the regular and severely exacting astronomical routine of observations, two centres of interest have been recently occupying the utmost activities of the institution; the reception, mounting, and use of the new great equatorial, and preparations for the transit of Venus.

The great equatorial has but one near approach to itself in the diameter of its object-glass—that of the private establishment of Mr. R. S. Newall, at Gateshead-on-Tyne, whose telescope has an object-glass of 25 in. in diameter. The Naval Observatory glass has 26 in. clear aperture. It is not easy to realise what this power is, and what it promises. The reader must imagine himself within a dome, itself 41 ft. in diameter and 40 ft. in height, looking through a tube made of three sections of steel stretching away for 32 ft.; the whole telescope and its metallic base weighing about 6 tons.

In the dome, on a pier of mason-work, supported by a pedestal, which is one block weighing 7½ tons, stands the fine equatorial made by Merz and Mähler, Munich, at a cost of 6,000 dols., its object-glass being valued at more than half that sum. The work of this instrument under, successively, Profs. Ferguson, Walker, Hubbard, and Hall, has been chiefly upon the smaller planets, the asteroids, and comets. Mr. James Ferguson was the first American to discover an asteroid, Euphrosyne, in 1854, the thirty-first on a list which has been recently enlarged beyond even a hundred by Peters of Clinton and Watson of Ann

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* Continued from p. 189.

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